OUR BOOK SHELF.

Directions for Collecting and Preserving Insects. By C. V. Riley. (Washington: Government Printing Office, 1892.)

DURING the last few years there has been in America a considerable increase of the number of persons interested in entomology. This may be due mainly to the fact that farmers have very practical reasons for studying insects, but no doubt it springs in part from a growing appreciation of the scientific aspects of the subject. However the increased interest is to be explained, one of its results is a constant demand, especially from correspondents of the U.S. Museum and the Department of Agriculture, for information as to how to collect, preserve, and mount insects. In the present work Mr. Riley undertakes to meet this demand. He also brings together a number of directions on points connected with such matters as the proper packing of insects for transmission through the mails or otherwise; labelling; methods of rearing; boxes and cabinets; and text-books. The work was prepared as a part of a Bulletin of the National Museum, but is also issued separately; and we need scarcely say that it is likely to be of great service to the class for whose benefit it was originally planned. Mr. Riley knows his subject so thoroughly that he is able to explain it simply and clearly, and the value of the text is enhanced by a large number of suitable illustrations. We may note that, in a paragraph on the scope and importance of entomology, he refers to various estimates of the number of insects in the world. Linnæus knew nearly 3000 species. In 1853 Dr. John Day thought there might be 250,000 species on the globe. Dr. Sharp's estimate thirty years later was between 500,000 and 1,000,000. In 1889 the estimate formed by Sharp and Walsingham reached nearly 2,000,000. Mr. Riley thinks even this estimate too low. Considering that species have been best worked up in the more temperate portions of the globe, that in the more tropical portions a vast number of species still remain to be characterized and named, that many portions of the globe are entomologically unexplored, and that even in the best worked-up regions by far the larger portion of the Micro-Hymenoptera and Micro-Diptera remain absolutely undescribed in our collections, and have been but very partially collected, he is of opinion that to say there are 10,000,000 species of insects in the world would be "a moderate estimate."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Science and the State.

IN last week's NATURE I find the statement that I was allowed to leave the public service "without the slightest recognition" by the State.

However distasteful it may be to me to have anything to say on this subject, I feel bound, in justice both to Lord Salisbury's and to Mr. Gladstone's former Governments, to point out that it is incorrect. Very substantial recognition was awarded me by both; and the late Lord Iddesleigh, in offering to recommend me for a Civil List pension, expressly put it as an honour.

The distinction which the Queen has recently been pleased to confer upon me must therefore, I am afraid, be placed in the category of "unearned increments." T. H. HUXLEY.

Barmouth, Wales, August 30, 1892.

[We did not refer to such recognition as is implied in the granting of pensions. What we meant was that the State ought to have marked its appreciation of Prof. Huxley's great services by conferring on him some national distinction of the kind he has now received.—ED.]

An International Zoological Record.

ON this subject Mr. Minchin (NATURE, August 18, p. 367) writes as a Recorder, and he writes feelingly. Those who use Records can write with feeling too. The absurd waste of labour involved, even in the production of a single Record, by the present system is hardly to be excused by the consideration that the labour is voluntary. I say "voluntary" advisedly, for some three or four pounds is no pay for a month's hard work. And yet, for all this toil, the result, when, after a year or so of delay, we are presented with it, is notoriously unsatisfactory. It is indeed impossible for a single individual—often very far from acquainted with the subject he is recording—to work through all the scientific literature of the whole world for the preceding year, in search of some scattered references. Actually impossible, for the literature of one year never comes completely to hand before the end of the next, and perhaps not then; and this the Recorders seem to know, for many of them postpone their work till the autumn, though it should have already been published in the spring. The acceptance of Mr. Minchin's admirable suggestions would do away with the ridiculous decimiplication of labour, but it would neither make the Record complete nor hasten its publication. The public are probably more anxious for the latter results than they are for the relief of the Recorders.

Almost absolute completeness, a higher standard of work, and greater expedition, would probably be attained by some such organization as the following:—In each country a Bibliographer, possessing an all-round acquaintance with the subjects to be recorded; this bibliographer simply to record, on separate slips, titles and places of publication of papers issued in *his own* country (and therefore probably in his own language), and to mark by some symbol the groups of animals or facts alluded to in those papers. An Editor-in-chief, situated in some convenient postal and printing centre, *e.g*, Naples, London, New York, Berlin, Paris, or Washington; this editor to govern the general plan of the Record, at present somewhat anarchic, to sort and distribute to specialists the slips which he receives from the bibliographers, and to edit the work. Lastly, for each group or division of a group, a Specialist, who, on receipt of the title-slips from the editor, should prepare the lists of new species, the abstracts of the papers, and a general review of advance in the subject. It may be pointed out that, by means of carbon-paper, title-slips can be easily written in duplicate or even triplicate; thus, by one writing of the bibliographer, slips can be prepared for the information of two or three specialists.

Such a scheme has the following advantages:—The literature is only gone through twice, instead of perhaps a dozen times. There is a possibility of completeness without much effort. Dates of publication can be ascertained with greater certitude. The quality of the work is improved by the employment of those specialists who will never consent to the colossal drudgery of the present system. Promptness of publication is possible. Thus, all slips for Europe and America could easily be sent to the editor within the first fortnight of the New Year, and by him transferred to specialists before the end of January. Literature from a greater distance would have to be sorted later on. Any specialist worthy of the name would already have seen most of the papers, and, with the help of abstracts from authors, could be ready with his manuscript before March, by which time the literature from the most distant parts would have come in and might be incorporated. The Record should go to press in separate divisions, so that the Birds need not be kept waiting because the Worms were not early; and the whole might well be issued in April or May.

The financial question must not be overlooked. As an International affair the sources of revenue of such a Record would be greatly increased. Not only Zoological and Royal, but also Geological Societies of all nations should be invited to contribute towards its expenses. The printer and the editor would have to be paid as now, the editor perhaps a trifle more. The postage would be a larger item, but postage is now so cheap that it really makes little difference. The bibliographers would of course have to be paid; but then the work of a bibliographer, even for the most prolific country, would be far less than the present work of the Recorder of, say, the Brachiopoda, who, I believe, gets about 30s. Some specialists would wish to be paid, but others would probably be satisfied by receiving the information from bibliographers and the abstracts and separate copies from authors. These latter, it is presumed, would gladly send single copies of their works for the use of a well-known specialist, but it is rather hard to have to distribute them to a dozen

© 1892 Nature Publishing Group

different Records, as one is requested to at present. The sale of such a work would be greater than that of the present incomplete and tardy publications. Besides, the promoters would doubtless be prepared to sell the various sections separately—an urgent reform that has long been clamoured for in vain; this alone would materially increase the receipts.

Having thought much of this subject during the last five years, and having talked it over with many Recorders and bibliographers, I venture to take this opportunity of putting forward the crude outlines of an undeveloped scheme. There is no wish to offend those unselfish toilers who have done and are doing so much for us, or the corporate bodies that support them. But this is a question that must be approached from a cosmopolitan standpoint. Men of science all the world over should support it with purse and person. All petty considerations of nationality, even of language, should be sunk. The aim of the work should be the advancement of science; only if it is truly International, can it possibly be realized out of Utopia. F. A. BATHER.

British Museum (Nat. Hist.), August 19.

PERHAPS you will kindly allow me, as the author of a certain pamphlet on "The Organization of Science," to say a few words on Mr. Minchin's letter (NATURE, August 18), which naturally had an especial interest for me. I am sorrowfully pleased to find the principles advocated in my pamphlet illustrated so well by concrete instance, and, needless to say, I heartily wish Mr. Minchin success in his endeavour to introduce order into at least one province of the scientific class, seeing that the text of my pamphlet may be exactly summed up in his remark—"A great need . . . is the intelligent *organization* of scientific research."

One point in Mr. Minchin's letter was of especial interest to me, for he invites the Royal Society to take in hand this work of organization, instead of leaving private individuals to execute at a great sacrifice the work which this wealthy corporation systematically neglects. Now a reference to my pamphlet (pp. 11-14) will show that this was a main thesis sustained there. Whether Mr. Minchin has done me the honour to read my pamphlet and is already preaching my crusade for me, or whether the similarity between our views is a simple coincidence of opinion, I know not, but whichever be the case, it is peculiarly gratifying to me to receive practically an endorsement from one whose experience renders him so especially qualified to speak with authority. A FREE LANCE.

London, August 23.

"The Limits of Animal Intelligence."

MR. DIXON has not, I think, quite grasped the main tendency of my paper read before the International Congress of Experimental Psychology. Nor is this to be wondered at. He quotes from a brief summary of what was itself but an abstract of a portion of a work on Comparative Psychology on which I am engaged. I am in agreement with nearly all that Mr. Dixon says, except where he misunderstands my position, and except in the opinion he expresses in the last sentence. When Mr. Dixon says, "Of course it is true that my knowledge of *my own* psychology does differ in kind from my knowledge of that of animals, but it differs in exactly the same way from that of all other men," he is expressing the views which I, in common with most men who have seriously studied the question, hold. And when he says, "If in no case is 'an animal activity to be interpreted as the outcome of the exercise of a higher psychical faculty if it can be fairly interpreted as the outcome of one which stands lower in the psychological scale,' the same rule should be applied to the (scientific) interpretation of human activities," I can only say that I heartily agree with him. Since, therefore, we have so much in common, I do not propose to occupy valuable space in discussing the outstanding points of difference between us. I may perhaps be allowed, however, to take advantage of the courtesy of the Editor of NATURE, and to say a few words in elucidation of the thesis I very imperfectly set forth in my paper, a thesis based entirely on observation and induction.

In the first place the study of my own mental processes, and of the nature and sequence of my own states of consciousness, has led me to the conclusion that there is a great difference between the mere feeling or awareness of certain relationships and the clear cognizing of these or other relationships. When I am bicycling, or playing tennis, or when I am living the practical life of naïve perception, I am aware of, and shape my actions in accordance with, a feeling of the relations which the objects of the external world bear to me and to each other. The greater part of my practical skill in action and of such intelligence as I show in meeting the emergencies that occur in my active life, are the outcome of this awareness of relations. But when I begin to attempt to *explain* phenomena, and to formulate my knowledge of the world, I find I am forced to pay special attention to these relationships *as such*, and to clearly and precisely cognize them. This conclusion, I repeat, is the outcome of observation, and is not, so far as I am aware, the result of any *a priori* considerations.

Looking back upon my own past, and collating the results with those reached by other observers, I find that the mere feeling or awareness of relations is prior in development to the clear and precise cognition of them. The awareness of relations seems to be, in fact, the undifferentiated germ from which their clear cognition has been developed; it is not knowledge, properly so called, but it is the raw material from which knowledge and the products of the intellect are shaped. Hence I conclude that the order of development or evolution in man is—first, the practical awareness of relations among phenomena, and then subsequently the cognition and clear knowledge (in the full sense of the word) of these relations as such.

Now, passing to the psychology of animals, such as the higher mammalia, the hypothesis suggests itself that they are still in the stage of mere awareness, and have not reached the stage of clear cognition, which, as I showed in my paper, involves reflection and introspection. This is put forward as an hypothesis; one based on observation and the doctrine of evolution; and one to be treated in the same spirit and on the same methods as other scientific hypotheses. It must be submitted to the touchstone of verification. The question is:—Are the activities of animals explicable on the supposition that the agents are merely aware of the relations; or must we suppose that they fully cognize them? I feel sure that my own practical activities are in the main based on awareness, and this leads me to suspect that the practical activities of animals are also of like psychological implication. The matter must, however, so far as is possible, be put to the test of experiment and observation. I have conducted from time to time experiments with the object of ascertaining how far there is evidence in the dog of true cognition—of causation for example. I am inclined to believe as the result of my observations that there is nothing beyond a simple awareness of the causal nexus. But I am far from wishing to dogmatize in the matter. I am chiefly concerned that the phenomena should be carefully observed, and that experiments should be conducted on definite scientific lines.

In conclusion I must be allowed to say that the phrases "difference in kind" and "difference of degree" savour somewhat of mere academic discussion, and may perhaps be left for those who deal with the matter on *a priori* lines and not from the standpoint of evolution. I for one do not for a moment question that the mental processes of man and of animals are alike products of evolution. The power of cognizing relations, reflection and introspection, appear to me to mark a new departure in evolution. But whether, as I am at present disposed to hold, the departure took place through the aid of language coincident with, or subsequent to, the human phase of evolution; or whether, as other observers and thinkers believe, it took place, or is now taking place, in the lower mammalia or in other animals, is a matter for calm, temperate, and impartial discussion founded on accurate, and, as far as possible, crucial experiment and observation. C. LLOYD MORGAN.

Rules of Nomenclature.

In your review of Mr. Massee's monograph of the Myxogastres (NATURE, p. 365) I notice the sentence, "Under the generally accepted rules of nomenclature, this leads to Massee standing as the authority for many species, transferred by him, in reality, to another genus." I take this to mean that, for example, a species of which the trivial name is, say, *abii*, and which was originally described by an author Xyz, and referred (erroneously) by him to the genus *Cdia*, has been transferred now to another (the correct, according to present knowledge) genus *Efia*, and the name is now printed in this work not as *Efia abii*, Xyz, but as *Efia abii*, Massee. I am aware that this course is frequently adopted, but surely not "under the generally accepted rules of nomenclature." There is no copy of the British Association "Rules" within reach here, but my recollection is that they

© 1892 Nature Publishing Group